service local, where the infringement of the law may have taken place.

The only element of discord in the Food Congress was developed by a proposal to establish an entirely independent food bureau, with an independent chemical laboratory, for the administration of the law. The head of this bureau, it was proposed, should be appointed by the President, for a term of four years, thus making the administration of the pure food law subject to frequent political changes. This provision was debated at great length for two days and at times with a degree of acrimony which indicated that some of the promoters of pure food legislation were more anxious to secure a new office than to establish a food law. At the end of this discussion the original plan, endorsed by previous Congresses, passed by a large majority and the bill thus approved was adopted without a dissenting vote.

On the third day of the Congress, by the invitation of the Committee on Interstate Commerce of the House of Representatives, a large number of the delegates attended the meeting of the Committee, at which the merits of the bill were presented in five minute speeches, by the representatives of the various industries attending the Congress.

In the interests of the public health and public honesty, it is to be hoped that the measure which has been recommended for the third time by this national Pure Food Congress, and which has the approbation of all the great trade interests of the country, will be pushed to a speedy vote and become a law before the present session of Congress adjourns.

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*THE ACCURACY OF THE EXPERIMENTAL METHODS OF THE CHEMIST.**

ON occasions like the present, where intellectual labor is the chief aim of those who are assembled together, it is a frequent practice for the speaker to offer to his audience something of the fruit which he has gathered from his own investigations. To the thought which lies at the foundation of this custom, are due those inaugural addresses which can lay just claim to being contributions to scientific knowledge. Other speakers prefer to furnish to these cultured circles a glimpse into the workings and tendencies of the higher institutions of learning. It were to me a great pleasure, esteemed colleagues and fellow students, could my efforts to-day draw me closer to you ; at all events this address shall serve as a greeting of welcome from the Rector, now entering upon his office, to all those who are connected with this *Hochschule*, and to all those who feel in it a kindly interest. And it is in this last sense that I beg leave to offer a few thoughts upon

THE ACCURACY OF THE EXPERIMENTAL METHODS OF THE CHEMIST.

It is the well recognized task of scientific investigation to discover the truth, and in those cases where this is not possible, to approximate it as closely as possible. In the natural sciences we may look upon this goal as attained when it is possible, in the broadest sense of the word *to describe* that which is appreciable to the senses. For this purpose it is generally insufficient merely to allow the object to act directly upon the senses, without in any way modifying its natural conditions ; we must avail ourselves of external assistance which either shall like a lens render our observations more accurate, or shall make possible the study of the object under changed outward conditions. On the one hand we use meter stick, balance, microscope ; on the other Bunsen burner, electrolytic cell, Röntgen apparatus, and the like. One need but cast a glance into the workshop of our investigator to see what an arsenal of appa-

* Inaugural address of the Rector of the Technischer Hochschule, Graz, Hungary. Translated by J. L. H.

ratus of both these classes stands at the command of the modern investigator in natural science; literally numberless are the conditions under which matter is ever anew compelled to yield up its secrets.

Let us now look more closely at the work of the chemist, that we may learn how far the results attained by the methods at his disposal may lay claim to exactitude.

The task which we have proposed may be treated in different ways. We could from a general standpoint, discuss the criteria by which the accuracy of an experimental method is to be judged. A method will thus be valued as exact according to the concordance of the results attained by it on many repetitions or according to the concordance of these results with those attained by other methods. The fulfilment of this last condition is especially of value, and we shall therefore later recur to an especially interesting case of this kind. The value of a method moreover will be further increased if it is fitted to broaden the field of experimentation; just as a piece of apparatus is of increased value, when it is adaptable for different experiments.

I prefer, however, to pass by these more general considerations and to discuss the exactitude of experimental methods in their bearing upon a few problems, whose fundamental importance is recognized. Of these I select three.

First we will consider the proof of the law of *indestructibility of matter*, as an example of the degree of exactness for which experimental data can be obtained, where the *determination of atomic masses* is concerned.

A second problem will be the *delicacy of chemical reactions*, under which it is to be considered, how far the accuracy of our apparatus allows us to detect the existence of definite chemical substances. And if both of these cases shall lead us to increased confidence in our methods, and we come to feel that the exactness of our experimental methods is very great, our third study on the other hand will serve to shatter this confidence. This study is that of *the absolutely pure substance*, and brings us at once to the point where the experimenter is wholly convinced of the justice of the conclusion that the hands of man can never bring into existence the unconditionally perfect.

Before we enter upon a consideration of these three examples, however, it will be well to notice the case which has already been spoken of, which reveals, as hardly any other could, the fact that those views, which have been attained by very different ways, and which we form from facts and phenomena upon the basis of sentient perception, may coincide to a remarkable degree; this is particularly the case if we take into account the possibility that our observations may not correspond to the reality quite as closely as the investigator commonly claims. In determining the density of the vapor of mercury, if one proceeds on the supposition that all gaseous bodies under the same conditions of temperature and pressure contain the same number of molecules in the same space, he comes to the conclusion that the vapor of mercury contains in the molecule only one-half as many atoms as that of hydrogen. If, as appears from the volume relation in the analysis and synthesis of hydrochloric acid, this hydrogen molecule contains two atoms, it then follows that in the vapor of mercury the smallest particles which exist in a free state are single atoms. Now it is possible to reach this result by a wholly different method. It is a familiar fact that the velocity of sound may be used to determine the relation between specific heats of gases at constant volume and constant pressure. For this determination the knowledge of volume and weight is necessary, and besides this a measure of length, that is the length of the sound waves. The ex-

periment furnishes us for this relation in the case of the vapor of mercury a higher value than in most gases. This ratio is one and two-thirds, that is, it is exactly the figure which is demanded by the kinetic theory of gases for a gas which contains a monatomic molecule, where the atomic energy of the molecule can be neglected when compared with that of the molecule as a whole. The velocity of sound in the lately discovered gases, argon, helium, neon, xenon and krypton, has also been found to correspond to monatomic molecules. This knowledge stands further in beautiful harmony with the chemical properties of these elements, whose atoms—apparently *without affinity*—appear unable to enter into any kind of combination. They seemed doomed to eternal solitude.

We now come to the first of the three problems which we have proposed for our consideration to-day. The question of the indestructibility of matter can best be introduced by recalling the investigations of the distinguished Belgian chemist, Stas, who attained an exactness in his work which has excited the admiration of all succeeding chemists. His investigations especially reveal where the experimental possibilities in this field find their limit. A closer view of his work would unfortunately compel us to enter upon a series of dry studies of figures. We will, however, take one example from among these. In order to prove our law, Stas prepared synthetically three quarters of a kilo of silver iodid, a quantity which the analytical chemist would consider enormous, and he found this weighed only fourteen and one-half milligrams less than the weight of the constituents of the compound. This is an accuracy reaching one fifty-thousandth part of the whole quantity. How extraordinarily close to complete agreement these figures lie may be judged when one considers that in our ordinary gravimetric methods, using perhaps a gram of substance, an error of one milligram may certainly be looked upon as small. This is an error of a one-thousandth of the whole quantity used.

As further experimental contributions to the law we are considering the work of Kreichgauer and that of Landolt is interesting. These investigators have carried on chemical observations in fused glass tubes in order to decide, by most careful weighing, these two questions: whether in a reaction, a ponderable portion of ether disappears or is added to the substances used; and whether the products of a reaction are influenced in exactly the same way by gravity as are the factors which enter into the reaction. Kreichgauer's experiments were calculated to detect a change in weight which would have amounted to one twenty-millionth of the mass present, but even this minute change of weight was not detected. In Landolt's experiments it was also impossible to detect any change in weight, although he did not consider the possibility of such a change absolutely excluded, chiefly because the variations, which were considerably less than one-tenth of a milligram, were always in the same direction.

In more recent times Ramsay and Rayleigh have carried out extremely accurate experiments. These distinguished scientific observers at first set before themselves merely the problem of determining, with the greatest possible accuracy, the weight of a few gases, among others that of nitrogen; but in the course of these investigations they came to the most unexpected results, for they were led to the discovery, and later to the separation of the unknown constituents of the air, argon, xenon, etc., as already mentioned. In such ways, an ever increasing perfection leads to ever more accurate results. And if it be true that in all investigations of this kind we must be denied the possibility of reaching the abso-

lute truth, there is, nevertheless, infinite satisfaction in the consciousness that we may approach nearer and nearer to the goal. The experiments of Ramsay and Rayleigh, however, lead us also to the knowledge that with the perfection of our methods, there may follow also the discovery of wholly new facts; and so investigation and experimentation are always worth while. They also bring as a reward answers even more numerous than the questions proposed. In other words, as in other fields, so also in natural science, one advance seldom takes place alone. In this sense it is not just that we should so frequently speak of notable discoveries being made by accident; for we thus minimize the labors and the deserts of the experimenter in the minds of others who are not familiar with his work.

We pass now to our second example. Methods which have for their aim the qualitative detection of elements are, other things being equal, considered more valuable according as they are more sensitive, that is according as they lead one to his desired end with the use of the least possible quantity of substance. In this respect it seems to be almost universally accepted that the smallest quantity of substance can be recognized, not by special experimental apparatus, but directly through one of our senses—that of smell. On the one hand this assumption rests upon the observation of E. Fischer and Penzoldt, that the limit of perception of the odor of mercaptan is reached when one four hundred and sixty-millionth part of a milligram of the substance is present. On the other hand the work of Kirchhoff and Bunsen shows that by spectrum analysis one fourteen millionth of a milligram of sodium can be recognized with certainty. By this it is not to be understood that Kirchhoff and Bunsen submitted this quantity as the smallest amount of a given element which will suffice to give a spectrum recognizable with certainty, but they merely put forth this statement to show the superiority of the methods of spectrum analysis in general. My own observations, the details of which do not belong here, have at all events convinced me that it is easy to so extend in spectrum analysis the limit of sensitiveness that we must to-day look upon spectrum analysis as by far the most delicate of all analytical methods. It is possible for us to compare the smallest amount of detectible substance with the hypothetical mass of the molecule, that is with the smallest quantity of substance that is capable of existing. In this way we easily reach the perhaps surprising fact that the least quantity of mercaptan perceptible to the senses contains about $2x10^{10}$ molecules. Hence, even of the most penetrating odors, it is necessary for twenty billions of their molecules to bombard the olfactory nerves, before these carry the sensation to the brain; they utterly ignore all smaller quantities. In ordinary analytical methods the quantity of substance is naturally incomparably greater. Without undertaking to weary you with figures I will in general and briefly note that by microchemical methods it is possible to detect about one ten-thousandth of a milligram, while in ordinary test tube reactions, it is desirable to have at least several milligrams of the substance present.

And now we come to the consideration of the last of the problems mentioned. The processes by means of which we decompose the mixtures which nature furnishes us, into their individual constituents, that is obtain from them chemically pure compounds, are of many kinds. We make use of variations in solubility and volatility, of the possibilities of crystallization and diffusion, of chemical, electrical and magnetic relations. These are, in a single word, really gradual differences, which can be used so much the less perfectly as the bodies to be separated resemble each other in phys-

ical and chemical aspects. Purification thus appears a process that can only be worked by degrees, so that we can consider the absolutely pure body wholly as a limiting condition. In the natural sciences it is often desirable to obtain as closely as possible such limiting conditions: for example we may mention the absolute zero, the possibility of which is clearly deducible by a simple consideration of the law of Gay Lussac: the absolute vacuum, a conception usually derived from the fact that the quantity of substance in a given space can be more and more diminished; (we are on the other hand convinced with certainty that it is as little possible to obtain a true vacuum artificially as it is to conceive of it in interplanetary space). Again, the condition of absolute equality of temperature between two bodies in contact, or of a system which is in absolutely complete chemical equilibrium, are purely ideal limiting conditions, which, practically speaking cannot be attained. In this sense it is not possible for us to obtain any substance completely free from impurities, and we must therefore be contented to carry the purification as far as possible—a task with which chemist and physicist are employed. A few striking examples may be noted in this field, and especially those in connection with the name of Stas. At first for the purpose of carrying out his atomic weight determinations there was demanded exceptionally pure substances, and it was naturally necessary for him to devise the means of obtaining them. Later he took up the experimental proof of the problem which had been suggested by Lockyer as to the composite nature of certain metals, and for this purpose Stas prepared with the greatest care a series of pure salts. For example, after eleven years' labor he succeeded in preparing a specimen of potassium chlorid, in which it was not possible to detect by any means the slightest trace of sodium. The investigations of Kohlrauch and Heydweiller may be mentioned here. They undertook the preparation of pure water in order to be able to determine its electrical conductivity with the greatest possible exactness. These investigators were obliged to carry out their work in glass vessels, and under such circumstances we can but be astounded that they succeeded in obtaining water so pure that a liter contained the exceedingly small quantity of a few thousandths of a milligram of foreign substance. More recently W. Spring has been engaged in this same problem of the purification of water, and has thrown new light upon it. He has found that in all liquids which have been purified by distillation (that is, according to the methods commonly in use up to this time) small particles are still present which may be detected by optical methods, and which in some circumstances can be eliminated by an electrolytic process.

Thus these new and more accurate methods furnish results which now in one direction and now in another can be considered more exact, although the attainment of the limit, as already mentioned, must be regarded as impossible. In other words, we are not in a position to absolutely exclude from a given space which is filled with one substance a large number of other substances. Nevertheless perhaps, in a single instance it has been possible. To touch upon this briefly, we must notice the problem which has been handled with remarkable accuracy by Baker, an Englishman. Baker has set for himself primarily the problem of the relations of gases to each other in conditions of the greatest possible freedom from moisture. In passing it may be stated that these gases were kept in fused glass tubes for months in contact with the best drying substances, for example phosphorus pentoxid, and thereby acquired in most cases very remarkable properties.

Substances which in other conditions react upon each other with the greatest energy were perfectly indifferent. Thus the dangerous phosphorus, which generally takes fire in the air at a temperature somewhat lower than that of the body, can be distilled in oxygen ; and hydrochloric acid and ammonia gas can be mixed without indicating any reaction. How shall we reconcile these observations with our commonly accepted theories? Is there here an interval in the properties, and is this interval connected with a similar interval in the conditions; that is, have affinity and moisture here in fact become absolutely nil? It is indeed hardly necessary to assume that the affinity has become zero, for one can equally well account for the phenomena, if one assumes that the velocity of the reaction has diminished very greatly, as seems already to have been proved the case at very low temperatures and for a perfectly dry mixture of carbon monoxid and oxygen. Also against the assumption that in Baker's experiment the last molecule of water has been wholly removed may be cited the fact that the water which is contained in the metaphosphoric acid still possesses an extremely small vapor tension. Further, even if Baker's dried gases really undergo a sudden change in their chemical properties, it does not necessarily follow that at the same time a sudden change in the moisture shall be present. As a matter of fact, we know that the properties of matter often change in a very unexpected way when they are only in the vicinity of such a limiting condition. The electrical phenomena in tubes of high vacuum present a familiar example. From a theoretical standpoint the facts which have been mentioned, aside from the interest which they arouse from their strangeness, are also remarkable on another ground. This has reference to the different hypotheses which have been proposed as the cause of reactions. Among these we may note that of Armstrong, who assumes the presence of free atoms or ions and looks upon the conditions of reaction in gases, practically in a similar way to that demanded by the theory of Arrhenius and Ostwald for liquid electrolytes. By this the analogy which has been suggested between these two states of aggregation, and which at present dominates the thought in the province of inorganic chemistry, is rendered complete We may add to this that the remarkable observations, according to which gases under the influence of Röntgen and certain other rays become conductors just as if electrolytes, lead to the same conclusion. It shows us again in what close relationship the perfection of methods and of the experimental resources on the one side, and advancement in knowledge on the other, stand to each other. Experimental science and theory are a constant stimulus to each other and bring forth their fruit side by side.

And now I have come to the end of my proposed task. I have indeed spoken only from the standpoint of my own field; but I believe that the conclusions which are drawn from these considerations may be so far generalized that they appear of importance for the technologist also. The sciences on which his work rests are applied mathematics and applied natural science. The former can do its work wherever the latter has by experiment laid a foundation, and because thus in the improvement of methods and appliances results are being attained which are of increasing value, the technologist may take an interest, we may almost say an egotistic interest, in the successful development of the pure natural sciences. One thing further, of a more ethical nature may be suggested by the fundamental similarity of the problems in both departments. Just as we chemists labor to come as close as possible

to the absolutely pure substance, or to some numerical value (atomic weight and the like), in the same way the machine builder strives for the conveyance of energy with the least possible loss, and the engineer for the lightest possible carrier.

And if indeed, the complete solution of these problems lies beyond the scope of our powers, if from the side of nature the inexorable 'No' stands against us; nevertheless, we have in our hands in the increasing accuracy of our methods of work the true philosopher's stone, by means of which we can ever come nearer and nearer to our goal. Let us look upon the creation of new methods of experimentation and the improvement of the old methods in our practical science, as one of the most important duties for those who seek for progress and see in the development of our powers of reason the foremost task of the cultured mind.

FRIEDRICH EMICH.

GRAZ, HUNGARY.

CORRESPONDENCE OF C. S. RAFINESQUE AND PROFESSOR WM. WAGNER.

IN hunting through the natural history material collected by the late Professor William Wagner in the Wagner Free Institute of Science, some fourteen years ago, I discovered several letters from the eccentric naturalist Rafinesque, together with a number of pamphlets written by him.

Professor Wagner had evidently taken up Rafinesque upon his return to Philadelphia from Kentucky with all the enthusiasm that a man interested in the development of the study of natural history must have for one who gave his whole time and all the money he could scrape together for the amassing of collections in every department of zoology and botany. It would appear from the context that Rafinesque had got into trouble (no unusual thing for him) and wished Professor Wagner to go on his note for the amount necessary to relieve him. This was promptly refused for the reasons given in Professor Wagner's letter, and Rafinesque writes to him on the 10th of April, 1840, as follows:

DEAR SIR:—I wish you will send me five dollars at y'r convenience for my 'Amenities of Nature' or at least One Dollar for the first Number that you have already had—that is the price. The value of Montford is $10. having 261 plates & with my notes $12. to 15; while Mantell is only worth $3.

I sell my works, my shells, my drawings and my services—I give them away sometimes to particular friends only, altho' I can hardly afford it.

Yours, &c,

C. S. RAFINESQUE.

Professor Wagner immediately replied on the 10th of April, 1840:

DEAR SIR:—Your note of this morning I found on my table on my return home at noon.

Your singular request to send you five dollars "at my convenience for your 'Amenities of Nature,' or one dollar for the one you say I have rec'd really surprises me. I now return to you unread, as my time has been otherwise much occupied that which you loaned me for my perusal and to which I never subscribed. If you have done perusing my copy of Mantell which, you informed me, you had read with interest & pleasure, you will please hand it to the bearer, as I wish to lend it to other of my friends. You inform me you sell your works, shells, drawings & services. I would beg leave to remark I have no occasion for any of them at present. You add "you give them away sometimes to particular friends only." I would add if you intend the remark for me you must know I never asked you for anything, neither have I ever received an atom of any of your property or effects, no not the most trifling, neither do I want them. I really regret my refusal yesterday to enter the requested security has produced an ebullition of feeling as your note indicates. I would have thought that your age and philosophy would have controlled your passions particularly after my explaining my reasons.

Yours, &c,

WM. WAGNER.

The reply from Rafinesque is dated April 12, 1840, and reads as follows:

MR. WILLIAM WAGNER.—The work of Mantell was delivered to your servant as you requested. I am used to disappointments—it was not an angry feeling but *sorrow* I experienced at your refusal;